exposed. The latest report of the U.S. Commissioner of Education deals with the year ending June 30, 1905. The total income for that year of American institutions of university rank, excluding benefactions, amounted to 8,355,000*l*., an increase of 289,200*l*. over the preceding year, and of this amount 23·6 per cent. was from State appropriations and 6·9 per cent. from Federal appropriations. That is, more than 2,506,500*l*. was provided from American public funds for higher education during the year with which the report deals. A very generous estimate of the amount provided here from public funds for higher education of every kind, including the Royal Colleges of Science of London and Dublin, the universities and the university colleges, would be to place it at a quarter of a million pounds sterling, so that the British case is in no way improved by importing the question of the amounts provided for higher education.

Sir W. T. Thiselton-Dyer is doubtful only about the greater belief of American statesmen in the need for the introduction of scientific methods in the solution of problems of government, but of the need of scientific ways of thinking on the part of our legislators he is quite convinced, and that is really the important matter.

A. T. S.

The Interpretation of Mendelian Phenomena.

APROPOS of the discussion on the interpretation of Mendelian phenomena, may I seek enlightenment on one or two points from your readers? Mendelian phenomena are possible only when reproduction is bi-parental. They cannot occur, of course, when it is parthenogenetic. I believe I am right in thinking that Mendelian workers suppose or hope that they have found a master key to the problems of heredity. Now, I am able to understand that the study of alternative inheritance may ultimately shed a light on the function of sex, but I find it difficult to conceive how it can shed a light on any other biological problem of importance; for example, the problems of the alleged transmission of acquirements, of the causation of variations, of the retrogression of characters which have lost selection value, and of the mode of development (whether or not by the recapitulation of the phylogeny). All these problems are of at least equal importance to the problem of sex. I have sought information from my Mendelian acquaintances, but I am always told that we must await the accumulation of data—a somewhat Micawber-like attitude, as it seems to me. I hope I make myself clear. The information I seek would be contained in the answer to the following question:—If Mendelism has a bearing on any biological problem save that of sex, what is that problem? If, as I anticipate, no one is able to name another problem, I venture to suggest that Mendelians are engaged in nothing more than the investigation of sex.

Mendelian phenomena have been observed principally in crossed artificial varieties of animals and plants. Crossed natural varieties usually blend their characteristics. This is conspicuously the case with man, the animal who, so far as is known, has crossed more often than any other, and whose hybrids may be observed up to the tenth or twelfth generation in South America and elsewhere. It has been said that "human skin-colour is the only character that is known to blend perfectly"; but this statement is certainly incorrect. With the exception of eye-colour, and possibly one or two other traits, such as the Mongolian eyelid, human hybrids appear to blend every character as perfectly as skin-colour. The transmission of no character is Mendelian. Thus mulattos have the black eye of the negro, and when they breed *inter se* continue to reproduce it indefinitely. There is no segregation. May I mention one other fact which is of considerable interest, but which seems to have escaped the attention of Mendelian workers? Crossed artificial varieties usually reveal latent characters in abundance. I am aware that the correctness of the term latency has been disputed, but it will serve to indicate what I mean. So far as I have been able to ascertain, no single instance of a latent character resulting from the crossing of natural varieties has been recorded. Certainly crossed human varieties reveal no such traits. A very significant passage bearing on this matter may be found in "Animals and Plants" (vol. ii., pp. 24–5). It would appear, then, that characters become latent only under conditions of artificial selection, that is, when mutations are selected. It has been maintained that nature also selects only mutations, but, to say the least, this has not been demonstrated as yet.

Bearing in mind, then, the facts that latent characters appear only when artificial varieties are crossed, and that crossed natural varieties usually blend their characters, the question arises whether Mendelians, so far from investigating even the whole problem of sex, are engaged in anything more than the investigation of those abnormalities of sexual reproduction which occur under conditions of artificial selection.

G. ARCHDALL REID.

Southsea, September 17.

On Correlation and the Methods of Modern Statistics.

PROF. KARL PEARSON's letter in NATURE of September 19 gives me a welcome opportunity of explaining what was not intelligible in the condensed report of my remarks in the discussion at Leicester, on methods of modern statistics.

Prof. Pearson communicated to the Royal Astronomical Society (Monthly Notices, May, 1906) a paper by Miss Winifred Gibson, giving an account of a research conducted in the statistical laboratory of University College, London. The first part of this paper discussed the relation between parallax and magnitude of the stars. I confessed to some misgivings as to the astronomical value of the results, and raised two questions, first, as to the method, and second, as to the matter.

Prof. Pearson thinks that I am on safer ground in the second than in the first. I will therefore examine first his reply to my second point, which was that the parallax material contained in Newcomb's table (appendix to "The Stars") is quite unsuitable for discussion by a general statistical method, since it relates very largely to stars selected for investigation because of abnormal proper motion.

Prof. Pearson "fancies that astronomers have been guilty of a considerable amount of circular reasoning. They start from the hypothesis that magnitude is very closely related to parallax. . . . The fundamental hypothesis that the brighter stars are much the nearer as yet awaits statistical demonstration. . . . Surely the hypotheses of high relationships between magnitude and parallax and proper motion are of sufficient importance to deserve *proof*, rather than to be taken as axiomatic." In this matter Prof. Pearson is under a misapprehension. Astronomers do not believe that magnitude is very closely related to parallax; very obviously it is not. But they do believe that parallax is somewhat closely related to proper motion. There are seventeen stars in the sky brighter than mag. 1·5, and their parallaxes have been determined with the heliometers at the Cape and at Yale. Here are the results.

Seven have proper motions (on a great circle) less than 0″·1 per annum:—

	P.M.		Parallax		Mag.
	″		″		
Canopus	0·00	...	0·00	...	1·0
Deneb	0·00	...	0·00	...	1·3
Rigel	0·01	...	0·00	...	0·3
Betelguese	0·03	...	0·02	...	0·9
Antares	0·03	...	0·02	...	1·3
Achernar	0·09	...	0·04	...	0·5
β Centauri	0·09	...	0·05	...	0·8
Mean ...	0·04	...	0·02	...	0·6

Six have proper motions between 0″·1 and 1″·0:—

	P.M.		Parallax		Mag.
	″		″		
Aldebaran	0·19	...	0·11	...	1·1
Regulus	0·27	...	0·02	...	1·3
Vega	0·36	...	0·11	...	0·1
Capella	0·43	...	0·09	...	0·2
Pollux	0·64	...	0·06	...	1·2
Altair	0·65	...	0·23	...	0·9
Mean ...	0·42	...	0·10	...	0·8

Four have proper motions greater than 1″·0:—

	P.M.		Parallax		Mag.
	″		″		
Procyon	1·25	...	0·30	...	0·5
Sirius	1·31	...	0·37	...	−1·4
Arcturus	2·28	...	0·03	...	0·3
α Centauri	3·67	...	0·75	...	0·2
Mean ...	2·13	...	0·36	...	−0·1

It is clear from the first group that low magnitude alone has no very close association with large parallax, and in the other groups Arcturus is the only star with considerable proper motion that has not a considerable parallax, as parallaxes go.

I think that in forming his low estimate of the established relation between parallax and proper motion Prof. Pearson must have been misled by Miss Gibson's results. She has calculated the correlation between parallax and the *separate components* of the proper motion in right ascension and declination, and so has naturally obtained a smaller result than if she had used the whole proper motion in seconds of arc on a great circle, as is usually done.

I have plotted the material used by Miss Gibson in two ways, parallax against magnitude and parallax against proper motion on a great circle, and it seems to me that the second diagram shows considerable correlation.

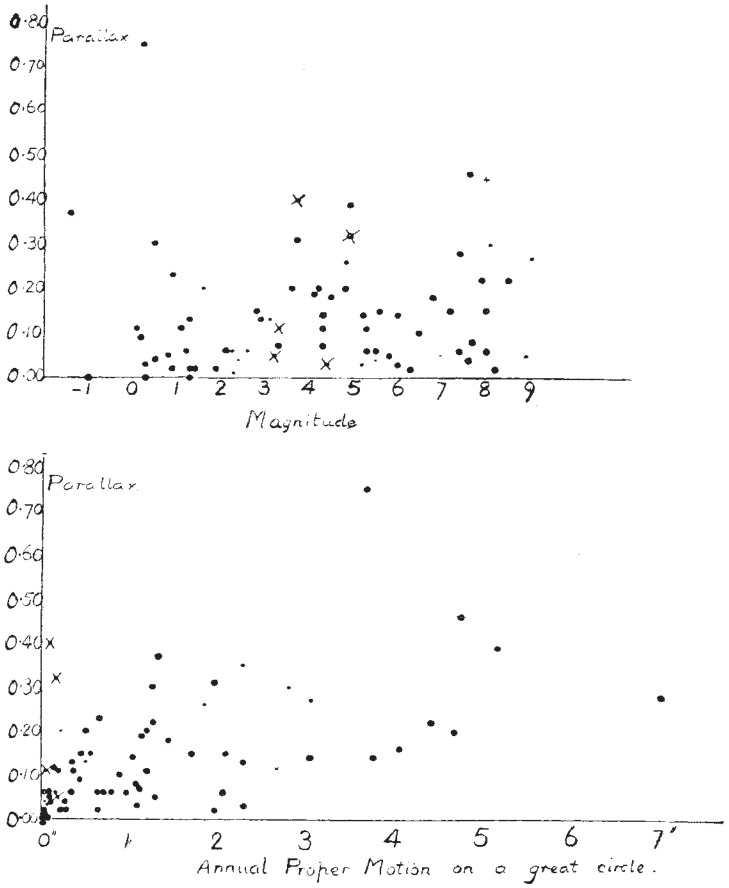
Some of the parallaxes are marked by Newcomb as doubtful; these I have shown by smaller dots. Five which depend on absolute right ascensions observed with the meridian circle may be considered too doubtful for use in this discussion; I have struck them through with a cross.

The seventy-two parallaxes grouped in means by whole magnitudes give a "curve of regression" with three peaks and two valleys, to which Miss Gibson thinks that it might be possible to fit a quartic curve. I suggested a simpler explanation, and Prof. Pearson asks for details of the reason why this curve of regression has such a switchback form if it is not real. It is easy to answer this question. Consider, for example, the second-magnitude group; it contains six stars, only one of which has a moderately large proper motion. Four of the stars were observed in the early days of photography as part of the late Prof. Pritchard's scheme to determine the parallaxes of all second-magnitude stars. One of the stars is Polaris, of small proper motion, observed just because it is Polaris; the sixth is α Gruis, of small proper motion and parallax. The group contains no star specially selected in anticipation of finding a large parallax, and naturally the mean of the group is small, because it is more or less representative of the second-magnitude stars in general.

On the other hand, the fourth-magnitude group includes several stars observed because of large proper motion, η Cassiopeiæ, τ Ceti, 70 Ophiuchi, and is helped out by a large parallax assigned to η Herculis which is certainly spurious. Hence the mean parallax of the group is large.

And so on. It would be tedious to examine each group in detail, but the almost fortuitous value of the mean parallax for each of these small magnitude groups is amusingly illustrated by the first two. The first contains two stars, parallaxes 0″·37 and 0″·00; mean, 0″·19. The second contains five stars; four have parallaxes of 0″·09, 0″·00, 0″·03, and 0″·11, but the group is translated from the valley to the peak because the fifth parallax is 0″·75. This, the largest known parallax, belongs to the double star α Centauri, the components of which have magnitudes 0·4 and 1·9. We might add the fainter component to the second-magnitude group discussed above; this would bring the mean parallax of the group up with a bound from 0″·08 to 0″·18, make a new peak, and remove one of Prof. Pearson's difficulties.

Probably there is no need to justify more minutely my remark at Leicester that a statistician who attacks astronomical problems must be as intimately acquainted with technical astronomy as with the use of his modern statistical tools. If any doubt remains, it will be dispelled by reading further in Miss Gibson's paper. "The next stage in the work was to inquire whether magnitude was more markedly correlated with any other character than parallax. Colour suggested itself as a character for which definite data were available. In vol. ix. of the 'Annals of the Cape Observatory' will be found a catalogue from which the colour of 159 stars can be extracted. . . ." Prof. Pearson quotes in his letter one of the results of this section, "that colour and magnitude are related at least as closely as parallax or proper motion and magnitude." And Miss Gibson concludes "that we have a suggestion, even if it be only of the vaguest kind, that the bulk of the lucid stars may belong to a separate universe within which magnitude is not mainly determined by parallax or distance, but is more closely associated with colour, and thus probably with chemical or physical conditions." On examining the basis of this far-reaching suggestion about the "bulk of the lucid stars," one is struck with the remarkable fact that "white had no frequency in the record"! A search in the Cape volume shows that the list in question is entitled "Coloured Stars observed during the Revision of the 'Cape Photo-



Correlation between (1) parallax and magnitude; (2) parallax and annual proper motion on a great circle, for the seventy-two stars in Newcomb's Table.

graphic Durchmusterung.'" This revision was chiefly an examination of all cases in which stars found in other catalogues are missing from the C.P.D. Sometimes they were hidden by photographic defects; sometimes they were variable or had large proper motion; in a great many cases they were red, and photographically faint. Hence attention was concentrated upon a particular class of stars, near the limit of the C.P.D., which had escaped registration owing to some abnormality, generally redness. This specially selected material has been further specialised, for at the top of the list we read:—*Very few stars of a lighter shade than deep yellow have been recorded because of their frequency.* And this is the material from which Prof. Pearson and Miss Gibson extract their conclusions about the stars in general.

I think that an astronomer may be allowed to express his dissent from these applications of modern statistical methods.

I should like, in conclusion, to refer briefly to the theoretical point:—Should not the correlation ratio be used in preference to the coefficient of correlation? The former expresses the degree of approximation to a smooth curve, the latter the approximation to a straight line. It will rarely happen that the curve of regression for two absolutely correlated astronomical quantities is a straight line, and the range of the quantities is generally so large that we are concerned with much more than an elementary portion of the curve. It appears to me, therefore, that it would be better in all treatment of astronomical statistics to use the correlation ratio. ARTHUR R. HINKS.

Cambridge Observatory, September 23.

The Relation of Man to the Animal World.

IN the short notice given of my "booklet" in NATURE of September 12 on the "Relation of Man to Animals," the writer seems quite to have misunderstood the object of my writing it. I am aware that an author has no right to discuss a fair criticism with a reviewer, but in the present case, when the latter says that he is at a loss to discover the object and aim of my pamphlet, I trust you will allow me to give its true explanation to your readers.

I may state in a few words that if we read the report of the Vivisection Committee which has been lately sitting, we shall see that there are witnesses who confess their ignorance of the many kinds of cruelty being inflicted on animals in their very midst. This ignorance has always been considered a very important point with the advocates of experimentation as showing that the anti-vivisectionists shut their eyes to everything connected with the use of animals except that which is for the attainment of knowledge. This knowledge some of their leading advocates declare is no value.

When I was chosen by my college to give an address on the subject before the Church Congress, I made the present position of the animal world my basis of argument for experimentation. This feature of the question is considered most important at the present time, and therefore I have very willingly re-published and expanded it in my pamphlet. This is my sole object of writing it. It would be quite out of place to give my own opinion as to our treatment of animals generally; in fact, it would be of no value considering the difference of opinion on the subject, and especially at the present time, when vegetarianism seems largely increasing.

Hampstead, September 27. SAMUEL WILKS.

Meteoric Shower, from near β Aurigæ.

ON September 27 at 9h. 54m. I saw a very swift meteor of about $1\frac{1}{2}$ mag. shooting through Cygnus, and leaving a streak of about 10° near α and γ Cygni. The sky was clouding over at the time, and a portion of the luminous course of the meteor must have been hidden, but the observed path was from $306\frac{1}{4}^\circ+45^\circ$ to $299^\circ+32\frac{1}{2}^\circ$.

The line of flight traced far backwards carries us to the point $88^\circ+43^\circ$, and this, I believe, formed the radiant position of the object. There are showers from near β Aurigæ in August, September, and October, and later months also supply indications of activity in the same centre. This particular system of Aurigids appears to be unusually rich from about September 21 to 27. On 1879 September 21 I recorded eight meteors from a radiant at $87^\circ+43^\circ$, and on 1878 September 25 the radiant near β Aurigæ was re-determined from five meteors. The members of this stream are exceedingly swift, and they usually leave streaks, their appearance being very similar to that of the Leonids. W. F. DENNING.

Bishopston, Bristol, September 28.

A New Stratigraphical Fact in the Thames Basin.

IT will interest geologists to learn of the occurrence in this neighbourhood, below the Tertiary plateau gravels, of a bed (not a boulder) of marly, lignitiferous, glauconitiferous limestone, full of fossils, about the Upper Eocene or Oligocene age, of which there can scarcely be a doubt. Fuller particulars are reserved for a later communication either to NATURE or the *Geological Magazine*, when the fossils have been exactly identified and the investigation completed.

This is, I believe, the first definite record in the Thames basin of pre-Miocene strata younger than the Upper Bagshot Sands. A. IRVING.

Bishop's Stortford, September 25.

Bees' Stings and Rheumatism.

IT appears to be a familiar and widespread belief in many countries that the stings of bees act both protectively and as a cure for "rheumatism." I have recently been able to collect some definite evidence in support of the belief in question.

This is of interest and importance, not only on general grounds, but also on account of the connection which I believe to exist between rheumatic fever and an abnormal production of formic acid (the acid of bee poison) in the human body (*British Medical Journal*, September 19, 1903, and May 25, 1907).

May I, therefore, be permitted to request any of your readers who possess information on the subject, and are willing to assist the present inquiry, to favour me with answers to the following questions?—

(1) Are you acquainted with the belief that bees' stings cure and prevent rheumatism?

(2) Do you know of any case in which rheumatism is believed to have been cured by this means?

(3) Do you know of any person who suffers from rheumatism although frequently exposed to the stings of bees? E. W. AINLEY WALKER.

University College, Oxford, September 27.

The Country Child in Education.

IN your interesting *résumé* of the educational meetings at the British Association in NATURE of September 12 (p. 505) one could not help being struck by the absence of any allusion, near or distant, to the *country* child.

I suppose it is correct to say that, physically, country children are, or will be, the backbone of the nation. If half as much were done educationally for them as is lavished on the town—and particularly the London—child, they would speedily become the nation's brain.

Manual training in all its branches, cookery lessons, swimming baths—what effect would these not have on our slow but sturdy village youngsters? With them nature-study is coming along—but even gardening lessons on scientific principles are all too rare.

Our school buildings and furniture in country districts slowly improve, but our playgrounds are still mostly loose beach, utterly useless for physical exercises or organised games.

It is time for enlightened educationists to urge the needs and claims of the country child. H. J. GLOVER.

Council School, Westham, Sussex, September 18.

[We hope this matter will not be lost sight of in arranging the agenda for next year's meeting of the Educational Science Section of the British Association.—ED. NATURE.]